

were seen near the radiant, and that they were generally smaller and had shorter tracks than the November meteors observed between 1865 and 1870. The number seen was too small to be called a shower; at the maximum, however, the fall per hour was nearly double that of ordinary nights. In short, I have no doubt that they were Leonids, and think it highly probable that they were derived from a distinct cluster which passed its perihelion in 1787 and 1820. We have therefore nine recorded meteor-falls which indicate the existence of a second cluster of Leonids, viz., those of A.D. 288, 855, 856, 1787, 1818, 1820, 1822, 1823, and 1852. The showers of 855 and 856 may be somewhat doubtful. If derived from the same meteor-cloud as the others, the dates would indicate considerable perturbations either by Uranus or the earth. The displays have been much less conspicuous than those of the major group, and hence the phenomena have been less frequently observed. The period is about 33.33 years, while that of the other swarm, according to Newton, is 33.25 years. Since their separation, therefore, the latter has gained nearly two-thirds of a revolution in their relative motion. The estimates which have been made in regard to the recent entrance of the cluster into the planetary system must consequently be rejected.

DANIEL KIRKWOOD

Bloomington, Indiana, U.S.A., April 20

Systems of Consanguinity

IN NATURE, vol. xi, p. 401, I find a notice of the third edition of Sir John Lubbock's valuable work on the "Origin of Civilisation," in which the following paragraph occurs:—"The facts with which he deals in this chapter [a new one in that volume] have been taken from the voluminous work of the American author, Mr. Morgan; but Sir John Lubbock, putting aside Mr. Morgan's theorising, has submitted a view of them of his own. This, in the main, and as far as it goes, we think, he has made out."

In the same article the following paragraph also occurs:—"One of Mr. Morgan's theories (for he has, or seems to have, two which it is no business of ours to reconcile with each other) is, that these systems are, to use the words of Sir John Lubbock, 'arbitrary, artificial, and intentional.'"

These statements, to the last of which with your permission I desire to reply, present the "American author" both harshly and unfairly to the British public. The interpretation of these systems of consanguinity, thus ascribed to me, is not mine; neither is the interpretation given in my work on "Systems of Consanguinity."

There are three or four places, and perhaps more, in that volume in which I speak of the system of a particular people as "artificial and complicated," and as "arbitrary and artificial," without the qualification in each case which should, perhaps, have been inserted. Thus, commenting on the same system (Con. p. 392), I remark that "the chain of consanguinity has been followed with great particularity, that the artificial and complicated character of the system might be exhibited, as well as the rigorous precision with which its minute details are adjusted." One who had read my work through could not have been misled by this statement, which was intended to characterise this system as it appeared on its face, and apart from all considerations respecting its origin. On the next page but one (p. 394) the same statement is repeated and qualified as follows: "As a plan of consanguinity it [the same system] is stupendous in form and complicated in its details; and seemingly arbitrary and artificial in its character when judged by ordinary standards."

In a single and final chapter of that work (pp. 467-510), entitled "General Results," I discussed the three great systems of consanguinity found in the principal families of mankind, and indicated some of the general conditions they seemed to warrant. My interpretation of these systems will there be found. To this chapter a person would naturally turn if he wished to know the views of the author on the precise question whether the systems were to be regarded as artificial or natural. Among other things, it contains what is prudently called a "conjectural solution" of the origin of the Malayan system of consanguinity, and also a similar solution of the origin of the Turanian system. These solutions are presented and discussed in connection with a series of fifteen prominent institutions and customs of mankind, articulated in a sequence in the order of their probable origination. It commences with "I. Promiscuous Intercourse"; "II. Intermarriage, or Cohabitation of Brothers and Sisters;" and ends with "XV. The Overthrow of the Classificatory System of Relationship, and the Substitution of the

Descriptive." In it are enumerated several successive forms of marriage, several successive forms of the family, and the three systems of consanguinity in their order of relation. It was designed to illustrate the course of human progress from savagery to civilisation; one form of marriage being followed by another, one form of the family by another, and one system of consanguinity by another. It is a sequence of human progress through the slow accumulations of experimental knowledge.

At the end of the solution of the origin of the Malayan system, which is founded upon the assumed intermarriage of brothers and sisters in a group (the second member of the sequence), occurs the following statement (p. 482):—"Every blood relationship under the Malayan system is thus explained from the nature of descents, and is seen to be the one actually existing, as near as the parentage of individuals could be known. The system, therefore, follows the flow of the blood, instead of thwarting or diverting its currents. It is a natural rather than an arbitrary and artificial system." The reader will notice that it was this form of marriage which created the Malayan system.

Again, at the end of the solution of the origin of the Turanian system, and after showing that the latter was derived from the Malayan, occurs the following statement (p. 486):—"If the progressive conditions of society during the ages of barbarism, from which this solution is drawn, are partly hypothetical, the system itself, as thus explained, is found to be simple and natural instead of an arbitrary and artificial creation of human intelligence."

In prosecuting this investigation one of the questions to be determined was whether these systems were artificial or natural. If the former, they are without ethnological value; but if natural systems, showing the relationships which actually existed when they were respectively formed, then they would possess immense value, because they concerned and demonstrated a condition of ancient society of which previously we had no definite conception. From each system, in such a case, can be deduced, with almost unerring certainty, the form of marriage and of the family in which it originated. It was by this course of reasoning that I discovered the necessary antecedent existence of the intermarriage of brothers and sisters in a group to account for the existence of the Malayan system of consanguinity. This fact gives us the starting-point in which ancient society commences, with the proof that it did so commence. Hence the second member of the sequence above-named. This sequence on its face, and these solutions in express terms, treat these systems as natural in every respect.

In an address before the London Anthropological Institute in 1871 upon the contents of the same volume on Consanguinity, Sir John Lubbock places me in the same position, and leaves me there. He remarks in that address (Journal of A. I., 1871, p. 6), which I presume forms the basis of "the new chapter," that "Mr. Morgan, from several passages, appears to regard the system as arbitrary, artificial, and intentional;" from which he takes occasion to dissent. I find in that somewhat elaborate address no reference whatever to the solutions named, and none whatever to the sequence. I am persuaded they must have escaped his notice.

LEWIS H. MORGAN

Rochester, New York, April 19

The Migration of Species

It has probably been the experience of most who have undertaken a voyage to sea, to have observed land-birds and insects far from the nearest coast, either in course of transit or resting on the vessel. Many travellers have observed these visitants, and their records have proved valuable biological facts bearing on the occasional migrations of species and their consequences as has been pointed out by Mr. Darwin. But it is more than probable that this dispersal of land species over extremely wide areas of sea is far more constant and less occasional than we are at present justified in affirming from the facts as yet collected. Unfortunately, however, we glean very little biological information from the great mercantile marine service of this country, an assemblage of which we are so justly proud, and it is only by costly Government expeditions that we become acquainted with facts that remained and would have remained unnoticed by the immense number of sailors who leave our shores. Nor can we feel surprised at the result when we recollect that biology is scarcely a subject thought necessary to form part of a mariner's education. A good instance is afforded by the results of the voyage of the *Beagle*. An impalpable powder fell upon the ship off the Cape de Verd Islands. This powder must have fallen upon many ships before; but Mr. Darwin being on board the

Beagle, it was collected and sent for inspection to Ehrenberg, and results of great scientific value accrued. Had our great philosophic naturalist not been there, this dust might still have fallen on ships to the present day, been swept away as a nuisance, and unrecognised as of any possible interest. That errant species must frequently visit vessels was shown me on a voyage to the East a few years ago. Thus, in the early part of September, in about lat. 12° N. and long. 26° W., a dove flew on board, which, after resting for a short time, again pursued its journey. In about lat. 9° N. and long. 25° W. a moth, apparently *S. convoluta*, reached the vessel just before the arrival of a squall. In reply to my inquiries, both the officers and crew stated that these were simply very common occurrences.

I think we may feel confident that most vessels sailing this course meet frequently with like objects, and the interest would be increased by finding whether the same were observed by vessels still further from the nearest land. Could some means be devised for obtaining records of these migratory species, or could some large shipowner be induced to have the same carefully recorded in the log-books kept on board his vessels, I feel little doubt that we should be astonished by the number and constancy of these wanderers from other lands. The entry in the log-book would ensure the date and approximate latitude and longitude which would be necessary factors in dealing with this biological question, and would doubtless bear further proof to Mr. Darwin's view of colonisation by chance or occasional visitants.

So much might be done by some of our present means of unendowed research that it seems weary waiting for the day when a broader education will tend to induce our sailors to reap that abundant harvest of scientific information which they so constantly have the means of acquiring. There is surely some branch of science which might be indebted to every vessel that sails from this country on a foreign voyage, could the preliminary information and impetus for inquiry be given to the officers or crew. I believe the "Religious Tract," or some kindred society, provides many of our vessels with devotional literature; could not our learned societies also compile and provide some scientific works and questions for solution which might be placed in the hands of our sailors, thus affording a pleasure for a long voyage, and producing effects to be appreciated by science at home?

We should not expect the results of a "Challenger Expedition," but then Government outlays for that purpose are sometimes few and far between.

W. L. DISTANT

Streatham Cottage, Buxclench Road, West Dulwich

Muraenopsis tridactyla

WITH reference to Mr. Kent's letter in your last number (p. 69), I beg leave to point out to you that it is very doubtful, according to the best authorities, whether the so-called *Muraenopsis tridactyla* is even specifically different from *Amphimu means* (i.e. the two-toed form of the same animal). Of the latter this Society have had several living specimens in their collection. One of them (purchased December 6, 1870) is still living in the Society's Gardens.

P. L. SCLATER

Zoological Society of London

Hardened Glass

THE account of hardened and malleable glass given in *NATURE*, vol. xi. p. 474, interested me greatly.

It seems hardly possible that a change in the molecular constitution of glass can take place without affecting its optical properties. May not this glass, therefore, possess refractive and dispersive powers unlike those of the kind usually employed in lenses? If it can be made of sufficient purity and is found to have a higher refractive power, it will enable us to make thinner lenses with smaller curves, thus contributing to the further improvement of optical instruments.

JAMES H. LOGAN

Jacksonville, Illinois, U.S.A., May 6

Yorkshire Exhibition "Guide"

WILL you kindly allow me, as a member of the Science Committee of the Yorkshire Exhibition of Arts and Manufactures, held at Leeds, to point out that the Yorkshire "Guide" referred in *NATURE*, vol. xii. p. 76, is entirely an unofficial publication. No competent member of the Committee was applied to for information respecting palladium or any other exhibit. The first intimation the Committee had of the wild statements

contained in the "Guide" was received from a member who purchased a copy in the usual way, and immediate steps were taken to secure that more trustworthy information should be contained in future editions of the "Guide," unofficial though it be. You will, I think, see that it is rather hard that the Committee should, as by inference they may be, be made responsible for the statements you indicate, and will, I hope, give me space for this repudiation of them.

H. POCKLINGTON

Primroses and Cowslips

IN answer to Mr. J. J. Murphy's inquiry in *NATURE* of May 13 (vol. xii. p. 34) I beg to state that the locality in which, as far as I am aware, no primroses are found, is formed by the outcrop of the chalk in the south of Cambridgeshire and north of Hertfordshire, and is bounded on the north and south by the outcrop of the chalk marl and the edge of the London Basin, and east and west by the Great Eastern and Great Northern main lines; it is, from the nature of the underlying beds, very dry. I have always thought, but perhaps without foundation, that primroses are not generally found in the districts in which cowslips are common, and *vice versa*, and Mr. Murphy's remark seems to bear out this.

I have not noticed any instance of the removal of the ovules of cowslips by birds; and even primroses, in other parts of the garden than those first attacked, have been left untouched.

Odsey, near Royston, Herts

H. GEORGE FORDHAM

OUR ASTRONOMICAL COLUMN

THE MELBOURNE CATALOGUE.—We have received the "First Melbourne General Catalogue" of stars, which is founded upon the observations taken with the Transit Circle under the direction of Mr. Ellery, the Government Astronomer, at the New Observatory of Melbourne, between the middle of the year 1863 and the end of 1870. It has been reduced and prepared for publication by Mr. E. J. White, the first assistant, from the materials printed in vols. ii. iii. and iv. of the Melbourne Observations. Vol. i. contained a catalogue of 546 stars resulting from the meridian observations taken previous to the removal of the Observatory to its present site, and called the "Williamstown Catalogue:" in the new publication we have the positions for the beginning of 1870, of 1227 stars, with few exceptions observed at least three times, and accompanied by the terms of precession to the third order, proper motions, and Bessel's reduction-constants (as in the British Association Catalogue), with the synonyms in Lacaille, Piazzi, Brisbane, and Johnson. Great care appears to have been taken in calculating the precessions from the mean year of observation to the epoch of the catalogue, and a detailed account of the process employed is given in the introduction. The proper motions of the stars have also been discussed where the means were available, the more uncertain results being distinguished from those possessing greater claim to acceptance by enclosure in parentheses.

Many of the most interesting stars of the southern heavens are included in this Catalogue, and we note that the remarkable one ϵ Indi has not been overlooked. In this case the recent Melbourne observations, as compared with Jacobs' at Madras in 1852, assign an annual proper motion of $4''.58$ in arc of great circle, thus quite confirming values previously obtained from less reliable data. We hope that at no distant period an attempt will be made to determine the parallax of this star. Large proper motion is indicated for the stars B. A. C. 5719, Aræ, and 7816, Indi; but on comparing the Melbourne positions with those in Gilliss's Santiago Catalogue, in the Washington volume of observations for 1868, not mentioned by Mr. White amongst the authorities he had consulted, it is not confirmed in either case.

The "First Melbourne Catalogue" is a handsome specimen of typography from the Government Printing Office. It must form an essential work of reference for every southern astronomer, who has now, with the "Cape